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ABSTRACT

Researchers have found that the labor market performance of exoffenders is dismal. The poor post-prison employment experiences of ex-offenders was investigated to determine whether their problems arose from being inexperienced low-skilled workers or from being ex-offenders. The sample was drawn from the Baltimore Life Insurance for Ex-Prisoners experiment, specifically males who had low financial resources, were repeat offenders, had no known history of alcohol or narcotic abuse, and had not been on work release for more than three months. Analyses of demographic data and employment patterns for one year following release from prison found a significant amount of covariance between experimental measures, making the isolation of the separate effects formidable. Previous employment experience was found to affect post-prison employment, but exhibited a weaker effect than experiences after prison. Criminal history was found to have a weak effect on post-prison performance. More recent unemployment consistently led to higher joblessness after prison. The finding that post-prison experiences were the most important determinates of employment success suggests that in-prison manpower programs may prove ineffective if the net result is continued confinement of inmates. (NRB)

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WORK EXPERIENCE, CRIMINAL HISTORY, AND POST-PRISON PERFORMANCE

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ABSTRACT

This paper explores whether the poor post-prison employment experiences of ex-offenders arise from general disadvantage as low-skilled workers with little previous work experience, or from specific disadvantage from being ex-offenders. While it is difficult to mesh out the separate effects, a strong finding is that monthly post-prison employment experiences are most directly affected by outcomes in months immediately preceding those months in question.

Work Experience, Criminal History, and Post-Prison Performance

Researchers have repeatedly found that the labor market performance of ex-offenders is dismal (Cook, 1975; Pownall, 1971; Taggart, 1972; Witte, 1976). The products of our nation's prisons experience high levels of unemployment, face high turnover due to dismissals, quits and layoffs, and receive low wages. Some researchers have argued that these conditions exist because ex-offenders possess in predominance low-skilled, disadvantaged worker characteristics. But is the dismal performance by ex-offenders in the labor market primarily due to their criminal record specifically or their disadvantaged status generally?

Phillip Cook (1975) has argued that the poor labor market performance of ex-offenders is due to their heavy endowment with characteristics associated with disadvantaged workers. They are young and nonwhite and hold unstable, low-paying jobs even before entering crime. While this situation may have pushed them into crime, having once been a criminal intensifies the disadvantaged worker effect rather than supplants it.

If Cook is correct, then among ex-offenders with varying previous employment experiences, the least disadvantaged should perform better. Disadvantage can be measured by not having held a job for any appreciable period, having worked the longest stretch in a poorly paid, low status, high turnover type job, or achieving only low educational status. And one would expect that, after prison, these measures would be highly correlated with failure in the labor market.

If Cook is not correct, on the other hand, varying post-prison unemployment experiences among ex-offenders should not be explained by

differences in these measures of employment disadvantage alone but perhaps by:

(i) varying criminal records, if employers discriminate against ex-offenders as ex-offenders; or

(ii) unmeasured characteristics that may reflect the degree to which the offender has been rehabilitated, such as high degree of motivation, sincerity, desire for the job, etc.

It is extremely useful to make a distinction between poor labor market performance due to general disadvantage as opposed to individual-specific phenomena, such as existence of a criminal record. The policy implied by the former is a broad provision of traditional manpower training and employment assistance. The policy implied by the latter requires specific remedies designed to address the particular categorical needs associated with conditions unique to certain individuals, for example, having a criminal record.

The substantive methodological problem arising from attempts to make such a distinction between general and specific disadvantage is that some of the hypothetical unmeasured characteristics may well be correlated with outcome variables, with the measures of disadvantage, or both. This problem is one familiar to labor economists studying state dependence and heterogeneity.

Researchers investigating the labor market experiences of individuals have observed that previous unemployment appears to affect the probability of becoming or remaining unemployed. Is this because being out of work causes potential employees to lose valuable work experience, making them

less productive and thereby less likely to be hired? Or is this because some unobserved characteristics, such as attitudes or motivation, affect the propensity to remain unemployed and by remaining constant through time, lead to a spurious correlation between current or future unemployment? The former case has been called State Dependence and the latter Heterogeneity. Chamberlain (1979) and Heckman (forthcoming) have pointed out that generally it is difficult empirically to differentiate between these competing hypotheses regarding the underlying cause of the observed correlation between past and current outcomes. A rough test of the hypothesis of no state dependence is, for example, a test that variables that do not change across spells of unemployment have statistically insignificant regression coefficients. This test is restricted to a limited definition of state dependence and appears less useful in analysis when the past is discontinuous, i.e., when there is a period of employment experiences prior to imprisonment followed by another period after release.

Lacking a rigorous statistical procedure for solving the Cook problem, we pose, instead, three interrelated questions:

(1) What effect does previous employment experience have on the post-prison performance of ex-offenders?

(2) Does it matter whether previous experience is before or after imprisonment?

(3) Are ex-offenders with more extensive criminal histories less successful in the labor market?

It can be hypothesized that if the poor labor market performance of ex-offenders does not come about because of general disadvantage but as a consequence of in-prison or criminal experiences, then post-prison

outcomes should have no (or a weak) relation to pre-prison employment. In addressing the above questions, we find very mixed evidence in support of the specific disadvantage hypothesis.

The paper is organized as follows. In section one the data upon which this study is based are described. In section two the effects of pre-prison and post-prison experiences or employment one year after release from prison are examined. In section three, these same effects are examined within the context of month-to-month post-prison employment outcomes. In a concluding section, we provide some tentative answers to the central questions of the study.

I. THE DATA

The Department of Labor sponsored an experiment in Baltimore between 1971 and 1974 wherein 432 high-risk male offenders were divided into groups that received weekly stipends of up to $60 a week for 13 weeks, got assistance in finding a job, or got neither or both. To minimize work disincentives, stipends were continued (but reduced) when employment was found until a sum of $780 had been received. The sample is drawn from the Baltimore Life Insurance for the Ex-Prisoners experiment (LIFE). (See Mallar and Thornton 1979.)

The sample consists of males released from Maryland's state prisons to the Baltimore Metropolitan Area who had low financial resources, were repeat offenders, had no known history of alcohol or narcotic abuses and had not been on work release for more than three months. While the average age was 24, 37% of the ex-offenders were under 21 years and only

10% were over 35. On the average, 4.387 years were served in prison for the current offense. Eighty-one percent had served 5 years or less. The range of time served was 2 to 21 years. About 87% of the sample was black, most had been raised in families with male heads ($\bar{x}_i$ = 67.8%), and most had jobs arranged when they were released from prison ($\bar{x}_i$ = 57.9%). However, a significant fraction had been previously arrested for disorderly conduct or were subsequently re-arrested for this crime ($\bar{x}_i$ = 17.6%). Most had held principally secondary labor market jobs or were previously unemployed ($\bar{x}_i$ = 52.5%), and all had extensive criminal records. The average number of previous arrests was 8 with 30% having 10 or more. The total number of arrests ranged to 40. Similarly, on the average the ex-offenders had been convicted 4 times with a range to 25 previous convictions.

Experience, denoted by the longest job held discounted by time since longest job held, averaged 17.5 months. It was calculated on the basis of the following formula:

Y = experience in months

X = length of time on longest job in months

Z = months since longest job

$$Y = X \cdot e^{-.004167(Z)}$$

The discount rate is approximately 5% per year.

Ten percent had had less than 2 months discounted experience, 30% less than 6 months, and about 50% less than a year. A group of 10% had had from 43 to 59 months of discounted experience. The average school grade completed was the 9th grade, and 60% had completed less than 8 years of school.

At the end of the year following release from prison, 61% had been unemployed an entire month for at least one month. Of these, 25% had only one month of unemployment, 23% experienced two months, 16% three months, 11% four months, and 6% five months and nearly 20% with one half of a year or more of unemployment. Moreover, almost 100 of the ex-offenders experienced more than one nonadjacent month of continuous spells of unemployment.

One year following release from prison, younger workers were more likely to have been unemployed the entire month, in jail, and/or sick than employed full- or part-time. There was no difference in the pre-prison arrest records of those who were unemployed the full month and those who worked full-time, although those who worked 21 to 35 hours per week had slightly fewer arrests while those who worked less than 24 hours per week had slightly more arrests than those who remained unemployed the entire month. More of those who were working 21 to 35 hours and those who were sick or in jail had some previous work experience than those who were either full-time workers or unemployed individuals. These results, along with other descriptors of the sample, are displayed in Table 1.

Although the average monthly full-time employment rate remained steady at two separate plateaus (at about 6% in the first six weeks and at almost 3% in the last six months) the month-to-month unemployment averages declined consistently with a few late-year exceptions. It should be pointed out that these figures are not adjusted for business cycle variations although the t^{th} month employment experience occurred for different individuals at different times during the year (since the reference point is date of release from prison).

TABLE 1

Description of Post-Prison Employment
Experience One Year After Release

	Employed (hrs./week)			Unempl. Whole Month	Not Employed	
	35	21-35	<24		Jail or Sick	Jail, Sick and/or Unempl.
Age	24.85	25.72	29.66	23.49	22	22.89
Previous Arrests	8	6	9	8	16	7
% Black	.87	.89	.67	.93	1.00	1.00
% Married	.13	.17	.17	.05	.30	.11
% with Previous Work Experience	.56	.67	.33	.51	.67	.33
% Raised by Persons Ever on Welfare	.35	.39	.17	.24	.53	.56
% of Family Members Ever in Prison	.39	.39	.17	.34	1.00	.78
No. Times Drank Liquor in First Week After Release	2.06	3.50	1.50	1.88	1.00	2.89
% Argued in First Week After Release						
with Father, Mother;	.04	.00	.00	.05	.00	.11
Brother, Sister;	.04	.00	.00	.05	.00	.11
Wife, Girlfriend	.13	.17	.00	.05	.33	.11

On the basis of a variety of measures of disadvantage (work experience, education, race, and arrest history), it is not surprising that we observe such extensive unemployment among those in the sample. But just as the degree of disadvantage varies widely in the sample, so too does the severity of the unemployment experience. It is legitimate, then, to ask of a sample such as this to what extent the variance in unemployment experiences is explained by varying degrees of pre-prison disadvantage.

II. PERFORMANCE ONE YEAR AFTER RELEASE

Four summary measures of post-prison performance were explored in an earlier study (Myers, 1980). The measures--a) full-time employment, b) unemployment, c) rearrest, and d) fighting--capture elements of both economic or social stresses encountered by those with imprisonment records reentering the outside world. Fighting with friends and relatives can be regarded as a form of social maladjustment and possibly as an antecedent to participation in crime. Rearrest can either be an indication of failure to be rehabilitated or very possibly a measure of failure in the criminal labor market. Both fighting behavior and criminal behavior plausibly could affect employment outcomes or themselves could be affected by employment outcomes. While on theoretical grounds these performance measures should be investigated simultaneously, a preliminary investigation revealed that there is at best a weak effect of the employment variables on fighting and rearrest. This suggests that it is appropriate to regard fighting and rearrest as exogenous. In the discussion that follows, then, the focus will be on employment outcomes as a means of post-prison performance.

Table 2 presents estimates of coefficients in a logistic model of the probability of being unemployed an entire month one year after being released from prison. Positive and significant are the effects of the number of previous post-release months of unemployment and the probability of having fought in the current month. Fighting was found in previous analysis to be strongly affected by living arrangements. It is seen in Table 2, though, that living with one's family tends to lead to lower probabilities of being unemployed. Because of the collinearity between fighting and living arrangements, it is difficult to discern the independent effects of these two important adjustment factors. The evidence is clear that unemployment is correlated with the occurrence of previous employment. While pre-prison work experience, age, race, arrest occurrence, or financial aid do not appear to have strong effects on umemployment one year after release, the number of months of either adjacent or separate spells of unemployment strongly influences the unemployment rate measured in this way.

Another way of measuring unemployment one year after release from prison is to compute the probability that in the first twelve months of freedom there is at least one entire month of unemployment. Of course, it is no longer legitimate to include post-prison unemployment occurrence as a separate determining factor. However, one would expect that in the absence of the dependence of the probability of unemployment on the occurrence of previous unemployment, time invariant factors would exhibit no independent, significant effects upon unemployment.

In Table 3, results of estimating a logistic model of the probability of being unemployed the entire month for at least one month after release are displayed. Older, more experienced workers are less likely to be

TABLE 2

Nonlinear Least Squares Estimates of Coefficients in Logistic Model of Unemployment During Month One Year After Release from Prison

	Coefficient	Elasticity
Constant	-3.5269 (-3.4112)	—
Previous Months Unemployed	.4347 (7.3632)	.8192
Fought this Month	2.0520 (5.2729)	.1179
Age	.0017 (.0519)	.0418
Experience	.0078 (.6171)	.1304
Living with Family	-1.1539 (-3.2620)	-.8819
Race	.0716 (.0895)	.0519
Treatment Group	.1144 (.3713)	.0546
Previous Months Arrested	.2869 (.9355)	.1319
$\hat{P}$ (predicted probability of unemployment)	.0457	

unemployed after prison, while blacks, those with more post-prison arrests, and those receiving financial assistance are more likely to be unemployed. By calculating the derivative of the odds against not being unemployed the entire month, it is found that the odds are that blacks are one and a fifth times more likely to be unemployed, and those in the treatment group receiving financial assistance are almost one half times more likely to be unemployed. (Each additional post-prison arrest increases the odds of being unemployed by about seven-tenths.) Each additional month of pre-prison discounted work experience subtracts 3/100 of a point from the unemployment odds while each year of older age at the time of release from prison subtracts 6/100 of a point.

It is seen in Table 3 that while there is a significant effect of time invariant variables upon unemployment outcomes, pre-prison employment has a relatively inelastic effect. Moreover, post-prison criminality exhibits a strong influence on the unemployment odds ratio. Thus, we cannot rule out the possibility of state dependence (a hypothesis we would reject if ex-offender, post-prison unemployment were caused by general disadvantage), nor can we rule out the possibility that there is an independent influence of criminal or prison experiences on post-prison performance (a hypothesis we would accept if there were specific disadvantage). In other words, general disadvantage may be less an impediment to employment of ex-offenders than specific disadvantage.

Another perspective on post-prison employment experience is gained by examining the probability of full-time work at least one full month in the 12 months following release. In Table 4, it is found that younger, more

TABLE 3

Nonlinear Least Squares Estimates of Coefficients in Logistic Model of Unemployment (t-statistics in parentheses)

Independent Variable	Coefficient	Elasticity	Odds Change[a]
Constant	1.1709 (1.5444)	.4443	1.9195
Months Fought	.0489 (.8149)	.0305	.0802
Age	-.0373 (2.8514)	-.3497	-.0611
Experience	-.0209 (-2.8514)	-.1399	-.0343
Race	.7256 (2.3799)	.2415	1.1895
Treatment Group	.2930 (1.3826)	.0556	.4803
Months Arrested	.4265 (2.0659)	.0858	.6992
Education	-.0506 (.9762)	-.1739	-.0830
Secondary Labor Market	-.0372 (.1681)	-.0074	-.0610

[a]The derivative of the unemployment odds ratio with respect to each independent variable.

experienced workers are more likely to be working full time as are those who received financial aid. Blacks, in addition, have lower probabilities of full-time employment. Post-prison arrest history, however, is insignificantly (although negatively) related to full-time employment. While the results of Table 4 do not provide strong evidence for the existence of specific disadvantage, the case for general disadvantage remains unclear when performance is measured by full-time employment.

III. MONTH-TO-MONTH PERFORMANCE

For each month a logistic equation was estimated for the probability of being unemployed the entire month. In Tables 5 and 6 the following results are displayed:

(1) Except in the first month, the effect of previous month's unemployment is strongly positive on current unemployment.

(2) Previous work experience has a negative and significant effect in only 5 months.

(3) The effect of criminal history is positive and significant at the 5% level in only 3 months.

(4) Being in the secondary labor market has an insignificantly positive effect for the first 3 months, positive and significant in the fourth, sixth, and eighth months, negative the intervening months, and ultimately negative and significant for the last 4 months.

TABLE 4

Nonlinear Least Squares Estimates of Logistic Model of Full-Time Work During Year Following Release from Prison[a] (t-statistics in parentheses)

Independent Variable	Mean	Coefficient	Elasticity
Constant	—	-.827755 (-.919539)	--
Months Arrested	.530093	-.028187 (-.124207)	-.012339
Age	24.708333	-.034157[b] (-1.390684)	-.696951
Experience	17.581019	.011737[b] (1.387987)	.170404
Race	.877315	-.428449[b] (-1.366127)	-.310409
Education	9.041667	.034845 (.578090)	.026018
Treatment Group	.5000	.332294[b] (1.398226)	.137208
Months Fought	1.643519	.089345[b] (1.409740)	.121259
Secondary Labor Market	.525463	-.084682 (-.341684)	-.036746
$\bar{P}$[c]	.224537		
$\hat{P}$[d]	.174192		
RMS[e]	.174		

[a]Significant at 10% level.

[b]The dependent variable is defined as follows: P = 1 if subject worked 35 hours per week at least one month in the year. P = 0 otherwise.

[c]Actual mean unemployment probability.

[d]Predicted mean unemployment probability.

[e]Root mean square.

TABLE 5

Nonlinear Least Squares Estimates of Coefficients in Logistic Model of Monthly Unemployment

(asymtotic standard errors in parentheses)

	U_1	U_2	U_3	U_4	U_5	U_6	U_7	U_8	U_9	U_{10}	U_{11}	U_{12}
Constant	.0456 (.6609)	-.1544 (.7105)	-1.813 (.7770)	-5.108 (.9580)	1.3460 (9.572)	-5.567 (1.005)	-6.3900 (.7366)	-.7782 (2.1060	-4.4412 (.9850)	-2.157 (.8542)	-2.964 (.8648)	-18.883 (1.464)
Experience	-.0153 (.0084)	.0101 (.0090)	.0010 (.0098)	-.0264 (.0107)	-.0367 (.0133)	-.0030 (.0110)	-.0244 (.0125)	.1113 (.0298)	-.0189 (.0110)	-.01118 (.0121)	-.0024 (.023)	.0134 (.0170)
Race	.8911 (.4326)	-.0206 (.4137)	-.0001 (.3947)	1.1110 (.5405)	.7230 (.6455)	1.1760 (.6752)	4.5780 (HC)	-4.0100 (4.6790)	.3688 (.5313)	-.2135 (.5213)	1.2540 (.4999)	-.3852 (.8453)
Treatment Group	.1660 (.2134)	.4786 (.2348)	.2467 (.2528)	-.3104 (.2661)	-.3479 (.3025)	-.1065 (.2655)	.0825 (.2741)	.2614 (.3747)	.6436 (.2988)	.6145 (.3009)	.6507 (.3131)	-.8558 (.4865)
Paroled	-.5968 (.2443)	-.2188 (.2614)	.0391 (.2832)	.7288 (.3258)	-.0649 (.3361)	.19180 (.3134)	-.0808 (.2961)	-.2074 (.3920)	.2410 (.3673)	-.2749 (.3227)	.4960 (.3281)	.7741 (.5471)
Secondary Labor Market	.0248 (.2194)	.0397 (.2420)	.1602 (.2529)	.6253 (.2806)	-1.0190 (.3453)	.9683 (.3015)	-.2238 (.3048)	1.4977 (.4405)	-.5236 (.3120)	-.4001 (.2909)	-.9841 (.3299)	-.7857 (.4464)
Job Arranged	-.5648 (.2217)	-.0959 (.2426)	-.7549 (.2658)	-.2861 (.2751)	-.2347 (.3043)	-.4366 (.2857)	-.9914 (.2994)	-1.3614 (.4390)	.9352 (.3824)	-.5454 (.2961)	-1.4980 (.3562)	-1.015 (.4408)
Age	-.0151 (.0207)	-.0373 (.0226)	-.0149 (.0267)	.0519 (.0246)	-.0480 (.0289)	.0527 (.0251)	-1.0130 (.0290)	-.3058 (.0967)	.0392 (.0747)	.0186 (.0293)	-.0540 (.0325)	-.0992 (.0633)
Previous Arrests	-.0163 (.0177)	-.0028 (.0185)	-.0077 (.0184)	.0228 (.0213)	.0396 (.0213)	-.0273 (.0226)	.0124 (.0290)	.0930 (.0363)	-.0189 (.0238)	-.0385 (.0276)	.0090 (.0261)	.3097 (.0396)
$Income_t$	-.0007 (.0006)	.0003 (.0010)	.0004 (.0005)	-.0001 (.0006)	-.0007 (.0010)	-.0001 (.0005)	-.0011 (.0007)	-.0036 (.0016)	.0003 (.0006)	-.0003 (.0007)	.0011 (.0006)	.0133 (.0040)
$Unempl_{t-1}$	10.6534 (7102.2000)	2.3406 (.3023)	2.290 (.2813)	2.645 (.3094)	3.5920 (.4024)	3.3000 (.3762)	2.927 (.3023)	9.7115 (4.579)	3.710 (.4281)	3.011 (.3305)	4.3461 (.4483)	19.916 (.00001)
Root Mean Squared	.213	.146	.123	.111	.090	.084	.090	.071	.086	.090	.056	.062

TABLE 6

Work Experience, Criminal History, and Post-Prison Unemployment[a] (t-statistics in parentheses)

Dependent Variable	Previous Month's Unemployment	Pre-Prison Work Experience	Total Arrests Before First Month
Unemployed in Month 1	10.6534 (.0015)	-.0153 (1.8214)	-.0163 (.9209)
Unemployed in Month 2	2.3406 (7.7426)	.0101 (1.1222)	-.0028 (.1514)
Unemployed in Month 3	2.2900 (8.1408)	.0010 (.1020)	-.0077 (.4185)
Unemployed in Month 4	2.6450 (8.5488)	-.0264 (2.4673)	.0228 (1.0704)
Unemployed in Month 5	3.5920 (8.9264)	-.0367 (2.7594)	.0396 (1.8592)
Unemployed in Month 6	3.3000 (8.7719)	-.0030 (.2727)	-.0275 (1.2168)
Unemployed in Month 7	2.9270 (9.6824)	-.0244 (1.9520)	.0124 (.5662)
Unemployed in Month 8	9.7115 (2.1209)	.1113 (3.7349)	.0930 (2.5479)
Unemployed in Month 9	3.7100 (8.6662)	-.0189 (1.7182)	-.0189 (.7500)
Unemployed in Month 10	3.0111 (9.1104)	-.0118 (.9752)	-.0385 (1.4051)
Unemployed in Month 11	4.3461 (9.6946)	-.0024 (.1951)	.0098 (.3755)
Unemployed in Month 12	19.9160 (161.8699)	.0134 (.7882)	.2097 (3.5185)

[a]Coefficients are obtained from nonlinear least square estimation of a logistic model of probability of being unemployed the entire month. Other independent variables included are: experimental group, race, secondary labor market, age, nonearned income in period t, job arranged, and released on parole. The dependent variable, unemployment in month t, equals one if the respondent was employed by the entire month and equals 0 otherwise.

With the exception of the secondary labor market wrong signs, these results can be regarded as weak evidence in favor of the general disadvantage view, especially if pre-prison work experience determines where one ends up in the first month's labor pool. For example, because of state dependence, after the first month subsequent unemployment may be determined by previous unemployment. This would imply that the covariance of pre-prison work experience and monthly unemployment is nonzero. Analysis of the monthly variance-covariance matrices suggests this is indeed the case.

However, the same results could suggest a heterogeneity argument. Ex-offenders may be partitioned within the labor market on the basis of some unmeasured set of characteristics. These characteristics are correlated with the propensity to remain unemployed for the entire month. Since these characteristics do not change from month to month, then current unemployment appears to be the cause of subsequent unemployment. In either case, the general disadvantage view is supported.

The wrong signs for secondary labor market deserve special comment. It is reasonable to assume that the insignificance of having been relegated to the secondary labor market before prison could be due to the experiment itself. Because of the provision of job assistance and financial aid,[1] the differences between secondary and primary labor market workers may temporarily have been obscured. Indeed, at the fourth month, when most individuals had received their entire stipend, the coefficient or secondary labor market jumps to a large positive value. There is, then, some instability of the signs, whereupon the strong negative effects are noticed in the last four months. It is unlikely that these negative

effects are due to the experiment. They are more likely due to the high turnover nature of secondary labor market jobs.[2] Such jobs as cook, sanitation worker, or parking lot attendant are not necessarily those for which unemployment during the entire month is to be expected. Instead, we would expect to find casual employment in these low-paying occupations. By using as the dependent variable unemployment the entire month, this aspect of labor market structure is not captured. Nevertheless, to the extent that being confined to the secondary labor market is a measure of disadvantage, one firmly committed to the general disadvantage view should expect to observe a consistent negative effect of previous dismal employment on post-prison employment.

When monthly full-time employment is tallied, as in Table 7, similar results emerge. Pre-prison employment experience is weakly related to the probability of working full-time during the month: in only one month out of twelve is the expected positive effect observed. The effect of secondary labor market is generally insignificant with unstable signs, although in the fourth month (when the financial assistance was exhausted) the effect is negative and significant. Only previous month's experience (being employed full-time the month before) has consistent and significant effects. The probability of being employed full-time in month t is positively and significantly affected by the probability of having been employed full-time in month t-1. Although these results should be qualified in light of the linear regression estimation techniques employed, even the most cautious conclusion would appear to be that pre-prison effects are less significant than post-prison effects.

TABLE 7

Work History and Full-Time Employment

	Pre-Prison Experience	Secondary L.M.	Previous Months' Experience
Release	.0006	-.0155	—
Month 1	-.0006	-.0125	—
Month 2	-.0007	-.0119	.5546[a]
Month 3	.0007	.0024	.4977[a]
Month 4	.0004	-.0325	.5623[a]
Month 5	.0005	.0113	.5766[a]
Month 6	.0000	-.0338	.5443[a]
Month 7	.0003	-.0172	.5479[a]
Month 8	.0000	-.0172	.4749[a]
Month 9	.0006	.0294[a]	.5554[a]
Month 10	.0004	.0069	.8393[a]
Month 11	.0011[a]	.0288[a]	.6921[a]
Month 12	-.0002	-.0105	.6591[a]

[a]Significant at 10% level.

Coefficients denote the derivatives of the probability of full-time employment in month t with respect to months pre-prison work experience, secondary labor market job status, and status of full-time employment the previous month. Estimates were obtained from a linear regression model wherein other independent variables were: job arranged, race, other income, experimental group membership, living with family, age, and parole status.

An important observation should be made about arrest history. Although this has been found to be highly important in determining annual unemployment, criminal arrest has only a minor impact on monthly unemployment. The measure of arrest history in the annual case, however, is the frequency of arrests subsequent to release from prison, while in the monthly case it is the frequency of arrests prior to prison release. Programming errors prevent the reporting of results of monthly unemployment using frequency of arrests subsequent to release from prison as a separate independent variable. Instead, in Table 8, estimates are provided of the effect of having been arrested in the previous month on the current month's unemployment. Similar, inconsistent, and insignificant effects are found. It may well be that different lag structures or nonlinear estimation would alter those conclusions in support of the general disadvantage view. But the monthly unemployment results, at odds with the annual results, do not suggest that frequency of arrests, either before prison or after prison, significantly affect post-prison performance.[3] At the same time, a finding of no affect of arrest record on unemployment among ex-offenders is <u>not</u> a finding of an absence of discrimination against ex-offenders as ex-offenders. This latter point is being explored in future research by the author.

IV. SUMMARY AND CONCLUSION

A convenient way to recapitulate is to search within the tangled web of analysis and results for the answers to the questions which motivated the analysis.

TABLE 8

Ordinary Least Squares Estimates of Coefficients in Linear Model of Monthly Unemployment

(T-statistics in parentheses)

Independent Variable	U_0	U_1	U_2	U_3	U_4	U_5	U_6	U_7	U_8	U_9	U_{10}	U_{11}	U_{12}
Experience	.0004 (2.1979)	-.0030 (3.2543)	.0008 (.3408)	-.0005 (.1470)	-.0025 (4.5285)	-.0015 (1.7567)	.0005 (.2461)	-.0009 (.7035)	.0012 (1.4918)	-.0025 (5.7407)	-.0003 (.0563)	-.0011 (1.4833)	-.0002 (.0339)
Race	-.0354 (8.6300)	.1616 (5.5123)	-.0192 (.1145)	.0004 (.0000)	.0950 (3.6585)	.0307 (.4417)	.0699 (2.5945)	.0955 (4.4185)	-.0232 (.3102)	.0724 (2.6193)	-.0214 (.2274)	.0842 (5.5165)	.0053 (.0196)
Flight$_t$	.0009 (.0114)	.0269 (.2377)	-.0189 (.1664)	-.0175 (.1314)	.0961 (3.9258)	.0039 (.0070)	.0058 (.0152)	.0244 (.2268)	.0082 (.0344)	.0647 (1.4270)	.0639 (1.1466)	-.0614 (1.4976)	.1904 (12.9140)
Treatment Group	.0083 (1.0805)	.0440 (.9601)	.0691 (3.4187)	.0376 (1.2009)	-.0599 (3.3396)	.0138 (.2087)	.0105 (.1340)	.0186 (.1319)	-.0067 (.0620)	.0333 (1.3635)	.0349 (1.3913)	-.0312 (1.7304)	-.0145 (.3338)
Released on Parole	-.0268 (7.1622)	-.1268 (4.9658)	-.0288 (.3730)	-.0000 (.0000)	.0515 (1.5517)	.0071 (.0340)	.0158 (.1938)	.0138 (.1368)	.0256 (.5599)	-.0277 (.5932)	-.0190 (.2614)	.0493 (2.7603)	-.0151 (.2293)
Secondary Labor Market	-.0007 (.0077)	.0081 (.0296)	.0057 (.0210)	.0615 (.2110)	.4166 (1.4784)	-.0397 (1.5600)	.0488 (2.6671)	-.0027 (.0072)	.0463 (2.6539)	-.0747 (6.1783)	-.0313 (1.0239)	-.0627 (6.2983)	-.0138 (.2689)
Arrest$_{t-1}$	--	--	.0659 (.2583)	-.1330 (3.0488)	.0360 (.2245)	.0327 (.1935)	-.0675 (.9042)	-.0835 (1.9735)	.0569 (.5982)	.0979 (1.4753)	-.0953 (2.0681)	.0541 (.8251)	.1027 (2.3779)
Unempl$_{t-1}$	--	--	.4183 (108.5522)	.3812 (89.8456)	.4187 (98.5603)	.5333 (174.4629)	.4996 (167.2716)	.4965 (133.1059)	.4846 (157.0995)	.5656 (166.1925)	.4828 (129.3634)	.5986 (309.7886)	.3882 (97.1432)
Job Arranged	-.0114 (1.6728)	-.1195 (5.7785)	-.0159 (.1479)	-.0800 (4.4607)	-.0275 (.5857)	.0728 (4.7333)	-.0559 (3.1209)	-.1059 (10.0776)	-.0489 (2.5589)	.0843 (6.2879)	-.0378 (1.3294)	-.0826 (10.1266)	-.03790 (1.8471)
Age	-.0003 (.1790)	-.0034 (.6937)	-.0052 (2.3128)	-.0026 (.6956)	.0051 (2.8886)	-.0053 (3.7414)	.0012 (.2209)	-.0013 (.2422)	-.0054 (4.7876)	.0019 (.5388)	-.0021 (.6175)	-.0011 (.2340)	-.0000
Constant	.0612 (7.7880)	.4794 (14.6638)	.2210 (4.4214)	.1977 (4.3052)	-.1005 (1.2357)	.2582 (9.7599)	-.05128 (.4188)	.0989 (1.4537)	.1695 (5.1156)	-.0065 (.0068)	.1825 (5.0099)	.0373 (.3282)	.0798 (1.3614)
R^2	.0522	.0903	.2403	.2247	.2346	.3358	.3188	.3046	.3189	.3078	.2544	.4603	.2529
F(8/423)	2.9121	4.6546	13.3176	12.2031	12.9067	21.2835	19.7040	18.4437	19.7173	18.7238	35.9059	14.2507	14.2507
U_t	.0069	.3543	.2500	.1921	.17361	.1713	.1435	.1551	.1273	.1412	.1389	.1204	.0949

Because of the difficulty of distinguishing between heterogeneity and state dependence in the data set, the answer to the core question is elusive. Whether the dismal post-prison employment experiences of ex-offenders in the Baltimore LIFE experiment are due to being disadvantaged workers generally or ex-offenders specifically is problematic.

This inconclusive result could have emerged under a variety of scenarios. First, and highly likely, is the possibility that both specific disadvantage and general disadvantage are so intimately intertwined that attempts to isolate one or the other weaken tests of the independent effects of either. To examine whether being in the secondary labor market, having poor skills and low education, or being black, generally, leads to career in crime, specifically, requires a data set including both offenders and nonoffenders. Of course, one could look at the effect of measures of general disadvantage on the rearrest rate of ex-offenders. In Myers (1980) it is found that pre-prison employment experience does not exhibit a consistently inverse effect on monthly rearrest rates. Although being in the secondary labor market is occasionally positively related to rearrest, for ten months out of twelve the effect is statistically insignificant. Nonetheless, in all of the results, there is a significant amount of covariance between measures of specific and general disadvantage making the isolation of the separate effects particularly formidable.

Second, and no less likely, is the possibility that there is not a sufficient amount of variation of disadvantage in the sample to adequately distinguish between specific and general disadvantage, let alone to detail how one affects the other. Although some of the ex-offenders had extremely long criminal records while others had only a few previous convictions,

there are no first offenders in the group. In addition, there is virtually no representation of highly educated, well-trained, successful criminals. Investigation of data sets such as those from Transitional Aid for Released Prisoners (TARP), which include first offenders, or from the U.S. Board of Parole, which include white collar criminals, may prove useful in this regard.

Third, and questionably, is the possibility that disadvantage, whether specific or general, is not directly a cause of the failure in the labor market by ex-offenders at all. The sometimes weak and often inconsistent effects of pre-prison work experience, secondary labor market status, and criminal history all may come about because none of these factors is really a determinant of post-prison labor market performance. The puzzle, then, is why are the effects of previous month's performance so strong, consistent, and robust? Is it perhaps because the lagged variable is capturing unmeasured aspects of disadvantage or unobserved correlates of the measured disadvantage? This, of course, is the central unresolved issue.

More conclusive are the answers to the three subsidiary questions posed. What effect does previous employment experience have on the post-prison performance of ex-offenders? When performance is measured by the probability of being unemployed one entire month for at least one month during the year after release from prison, experience is found to be inversely related to post-prison employment failure. Yet this effect is inelastic and results in only a small marginal change in the unemployment odds ratio. When performance is measured by full-time employment, the effect is positive, yet again inelastic. When monthly

unemployment is chosen as the performance measure, the inverse relationship between pre-prison employment experience and post-prison unemployment is found to be statistically significant in only five months and even then the marginal effects are small.

Does it matter whether what we call here previous experience is before or after prison? Yes. Regardless of how performance is measured, when both the effects of measures of pre-prison and post-prison employment are viewed together, the relative magnitude of the post-prison employment effects on performance is larger.

Are ex-offenders with more extensive criminal histories less successful in the labor market than other ex-offenders? Those with more post-prison arrests are more likely to be unemployed at least one month during the year following release and are less likely to be employed full-time (although insignificantly so) than the others. This supports the view that it is post-prison experiences that matter. Monthly unemployment performance, in contrast, is only weakly related to criminal history. The number of pre-prison arrests is positively and significantly related to unemployment in just three months out of twelve. Thus, while criminal history may matter, the more recent history is probably the more damaging for employability.

To summarize, previous employment experience <u>does</u> affect post-prison employment but pre-prison experience exhibits a weaker effect than the experiences had after prison. Similarly, criminal history has a weak effect on post-prison performance, particularly if one concentrates on the criminal history prior to release for the current offense. In Table 9, the relative magnitudes of the partial changes in the monthly

unemployment odds ratios due to previous month's unemployment, pre-prison employment and pre-prison arrest history are displayed. And it is clear that more recent unemployment experience consistently leads to higher joblessness after prison.

A tentative conclusion emerges that, while a strong case cannot be made against the belief that a cause of the poor labor market performance of ex-offenders is their heavy endowment with disadvantaged worker characteristics, indeed many elements of specific disadvantage, as ex-offenders, seem to impinge upon the successful reentry into the legitimate world of socially acceptable work. In particular, we could argue, some unmeasured attributes generated or inculcated prior to release from prison but affecting employment experiences immediately upon release from prison, may be operative. One can only speculate as to what these attributes may be. But this author has argued elsewhere (Myers, forthcoming) that criminal human capital accumulation may be associated with imprisonment, lowering the returns to work relative to the returns to crime. Moreover, the in-prison environment may generate worker characteristics that, while unobserved, may be--correctly or incorrectly--regarded by employers as inversely related to productivity. If this is the case, post-prison unemployment is a <u>state</u> that substitutes for an actual measure of those unobserved attributes and as such is used by employers to screen potential job applicants.

The finding that the post-prison experiences are the most important determinates of employment success or failure is important in its own right, even if one is unable to conclude why this finding arises. Certainly,

TABLE 9

Work Experience, Criminal History and Post-Prison Unemployment[a]

Dependent Variable	Partial Change in Odds due to Previous Months' Unemployment	Partial Change in Odds due to Each Additional Month's Pre-Prison Experience	Partial Change in Odds due to Each Additional Previous Arrest
Unemployed in Month 1	0	-.012	0
Unemployed in Month 2	3.86	0	0
Unemployed in Month 3	3.21	0	0
Unemployed in Month 4	3.65	-.017	0
Unemployed in Month 5	5.35	-.019	.054
Unemployed in Month 6	4.46	0	0
Unemployed in Month 7	3.75	-.016	0
Unemployed in Month 8	23.31	.787	.196
Unemployed in Month 9	4.82	-.014	0
Unemployed in Month 10	3.91	0	-.028
Unemployed in Month 11	6.43	0	0
Unemployed in Month 12	60.74	0	1.120

The partial change in the odds ratio is found by

$$\left.\frac{\partial\left(\frac{p}{1-p}\right)}{\partial x_i}\right|_{\beta_j = 0} = \beta_i e^{\beta_i x_i}$$

[a]Insignificant coefficients set equal to zero (10% level)

in making a choice between manpower programs for prison releasees and those for incarcerated offenders, policy makers would be forced to confront the possibility that absence from the labor market while incarcerated may have as damaging an effect on employment prospects as being unemployed the previous month does. Thus, in-prison programs, no matter how well designed, may prove to be ineffective if the net result is continued confinement of inmates.

NOTES

[1]Treatment Group is defined here as that group receiving financial aid, half of whom received job counselling. No separate test was made of the effect of job assistance, except that of having a prearranged job at release.

[2]The occupations classified as secondary labor market are Vendor; Cook; Waiter; Gas, tire worker; Warehouse packer; Factory worker; Custodial worker; Maintenance worker; Tree lawn worker; Government agency orderly, janitor, porter; Dishwasher; Construction laborer; Sanitation worker; Parking lot attendant; Other unskilled worker; and those never employed.

[3]It is also found in Myers (1980) that frequency of pre-prison arrests also does not affect post-prison rearrest. Moreover, frequency of post-prison rearrest is unrelated to post-prison fighting.

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